Foreword

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Much has been written by historians and sociologists as well as business commentators about the modern economy—the kind that supplanted the traditional economy in several nations in the nineteenth century and many more in the latter half of the twentieth century. The pre-capitalist system dominated by the self-employed and the self-financed gave way to finance capitalism. To call this a "great transformation" was no overstatement.

A traditional economy is one of routine. In the usual illustrative example, rural folk periodically exchange their produce for the goods of the town. The sole disturbances are not of their doing and are beyond their control—rainfall, temperature, and other exogenous shocks. This was the economy modeled in the neoclassical theory of economic equilibrium from Ricardo and Böhm-Bawerk to Walras and Samuelson. It is also the economy described in the subsequent stochastic models of "rational expectations equilibrium" in the face of shocks that were pioneered by Arrow, Samuelson, Muth, and Lucas.

The modern economy is marked by the feasibility of endogenous change. Modernization opens the door for individuals to engage in novel activity—most importantly, the financing, developing, and marketing of new products and methods. Furthermore, such innovations, when successful in the marketplace, have unforeseen effects on production possibilities, prices, the differentiation of goods, and the specialization of work.

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For decades, economics students have quietly asked themselves whether the equilibrium theory of the classroom is adequate for modeling the modern economy. It is one thing to know the prices at hand, another to know all prices far and wide and over the whole future and for every state of the world that shocks might land the economy in. Equilibrium theory implicitly takes the mechanisms that constitute the economy to be completely known: participants have a full understanding of how this organism works, so everyone knows the probability distribution of outcomes to expect in this or that state. This in turn implies that everyone knows this understanding to be common knowledge, so there is no diversity of views that would have to be guessed at in estimating what others intend to do.

History records a small band of economists who have called attention to points of dissonance between the modern economy and equilibrium theory, including the theory of rational expectations equilibrium, in which expectations (and thus prices) are taken to be appropriate for equilibrium in each possible current state.[1] In fact the growing perception, starting from the turn of the century, that the new modern economies were generally out of equilibrium, sometimes frighteningly so, is one of the hallmarks of twentieth-century thought.

The great interwar theorist at Chicago, Frank Knight, pondering the arrival of capitalism, took the unprecedented position in his 1921 classic *Risk, Uncertainty and Profit* that virtually all business decisions other than the routine ones are to an appreciable extent a step into the unknown. The possible outcomes might have probabilities but those probabilities were unknown, or "unmeasurable"—the radical sort of uncertainty now called "Knightian uncertainty." Viewing from London and Cambridge a similarly modern economy, John Maynard Keynes proposed in his 1921 *Treatise on Probability* a rewrite of probability theory that would take account of radical uncertainty. His 1936 *General Theory* was an attempt to overthrow equilibrium theory. In subsequent years, economists from Vienna to Copenhagen critiqued "perfect foresight" and its generalization, now known as rational expectations equilibrium.[2] In the glorious 1960s several American economists broke from equilibrium theory.[3]

1. I discuss some of the implications of this anti-equilibrium view for economic activity and inflation-unemployment tradeoffs in the context of my early expectations-driven macroeconomic models in my Nobel Prize lecture (Phelps, 2007).

2. I am thinking of the game theorists Morgenstern (1949) and Zeuthen (1955).

3. Ambiguity and vagueness were introduced by Ellsberg (1961) and Fellner (1961), personal knowledge by Polanyi (1958). Several applied papers followed in this spirit. For example, in a macroeconomic context, Friedman (1961, p. 449) pointed out that long and variable lags in the effects of monetary policy imply that forecasting the consequences of monetary action is "not an easy requirement in the present state of our knowledge." Phelps (1968a) argued that a (expectational) disequilibrium may be created by an undiagnosed structural shift.

Trained professionals in that decade had a sense of what this anti-equilibrium literature was getting at. We thought that, empirically, equilibrium theory would not work well. For one thing, the economies (at least the world economy) we lived in had become too rich for equilibrium theory to fit at all well: forming correct expectations about a sole experiment, such as a lower price or a new variation on a product, is one thing, but forming expectations when most or all firms are simultaneously experimenting is qualitatively different. For another thing, these economies were not really fluctuating around the stationary state or steady-growth path of neoclassical theory; they were constantly evolving in their structure and were changing too fast for economic relationships (between prices and quantities, for example) to have the durability that would be necessary for formation of accurate expectations about present and future data.[4]

Yet, a few years later, the community of macroeconomists, far from distancing themselves farther and farther from equilibrium theory, proceeded almost unanimously to embrace the rational expectations models of business activity introduced in the early 1970s. Keynes and the Cantabridgians were out. Hayek and the Austrians were out. So was Spiethoff and his German school.

This marvelous book by Frydman and Goldberg documents in its first two chapters invaluable insights of the "early modern" theory of capitalism that were lost when the profession endorsed rational expectations equilibrium. And it exposes to the light the arguments offered by the advocates of the premise of rational expectations. There can't be many readers who won't be fascinated by this story. In letting the two sides speak in their own carefully chosen words the authors allow the expressed points of disagreement to come into sharp focus.

These chapters, however, soon probe to a deeper level. It isn't just that the postulate of rational expectations is unrealistic in the same way that the postulate of rational choice is conceded to be unrealistic. We agree to work with rational choice in spite of its limitations, so why not rational expectations too? The primary issue is not an empirical one. Even if no firms at the current time were actually venturing a new price list, conceiving a new way to cut costs, devising new financial vehicles, contemplating a new product, and so forth, there would still be a problem: rational expectations equilibrium theory as an element of our models of the modern sort of economies contradicts the very essence of an economy in which economic actors are free to exercise their "creativity" by venturing to do something innovative.

4. The former argument is the theme of Frydman (1982). The latter argument is the theme of a recent paper of mine (Phelps, 2006a).

The authors argue that if we aspire to build models that apply to modern economies—economies whose central functioning is the manufacture of change through their innovative activity and their adoption and mastery of the innovations made available—it is contradictory to adopt the rational expectations postulate that whatever change takes place in the future is already knowable and known in the present: that the economic change to be experienced is in a sense predetermined. Yet contemporary model builders embracing rational expectations have been undeterred or unaware of the contradiction: they either specify that there is no change in the world (the world they would describe with their models) or that whatever process of change is going on in the world can be incorporated in their models in a fully predetermined way.

This criticism is not a narrow point that would be straightforward to remedy. The authors are not referring to the fact that the archetypal models of an economy enjoying rational expectations equilibrium have built into them an invariant trend-growth path to which the economy is constantly returning (as described by some transition dynamics). It is obvious that such a trend path is predetermined; the possibilities and probabilities are "prespecified" (in the authors' preferred term). The authors' argument is broader than that. If a rational expectations model supposed instead that the future was governed by a probabilistic linear birth process, so the model has no trend path to which the economy is tethered, there is still a fundamental predeterminacy: the possible states at a given future date are all known already and there is at present a calculable probability, conditional on the present state, of each such future state's occurrence. In this model too, then, there is implicitly no possibility for the actors in the economy to create something unforeseeable, surprising, genuinely innovative. Thus, there is a methodological choice: to model on the premise of fully prespecified future possibilities, which rational expectations requires, or to model an economy capable of endogenous change, which the modern economy is.

A recent case in point is the state-of-the-art model of the real business cycle type, where recognition is given to the accepted idea that opportunities are rosier at some times than others—the notion of "regimes" in which there are outsize rates of return in prospect for investment.[5] At first blush this construct appears to capture an economy undergoing the occasional boom and the occasional slump at unpredictable times and having a future that feels not fully predetermined—and all this without sacrificing the precision of rational expectations equilibrium. The truth is, however, that this is

5. See Beaudry and Portier (2004).

a highly mechanical apparatus implying a finite number of states at any future date and imputing to each a calculable probability conditional on the economy's present state.

An older case of equilibrium theory in macroeconomics is Joseph Schumpeter's great 1911 work *Theory of Economic Development*. He saw the need to go beyond the Spiethoff-Cassel model, in which no entrepreneur appears and none is needed, only the occasional discovery of an exogenous scientist or explorer. Forced to choose whether to remain with the equilibrium perspective of his idol Walras or instead to regard entrepreneurs as creators in their own—figures creating the future—Schumpeter clung to the equilibrium perspective. The Schumpeterian entrepreneurs were merely the vessels the economy needs to carry out the commercial innovations made possible by the technology. The stock of undeveloped innovations were all "in the air," each waiting for one of Schumpeter's "entrepreneurs" to find it convenient to take on its financing, developing, and marketing. The rate of return of every project was known, at any rate to the experienced banker. The Schumpeterian model makes determinate (at least probabilistically) the rate of innovative activity and the time path of productivity—as if the creativity is all science and no commerce.

In contrast, to elaborate on earlier remarks, Keynes saw the rate of return as quite unknown and the demand for investment funds as driven by entrepreneurs' "animal spirits." Hayek saw that every participant has little or no knowledge of how the economy works as a whole, contrary to rational expectations; that a participant is apt to have only some highly specialized knowledge about his or her industry, which is itself apt to be quite specialized; that in some cases it is so deep as to be "private knowledge"; and that such knowledge may permit a creative person to conceive some new business strategy or new business product that is *not* in the air, *not* already known by all. In the struggle between these two worldviews, Keynes and Hayek were right but did not carry the day.[6]

As the rational expectations view has come under increasing suspicion, parts of the profession have jumped to the conclusion that the problem is "sticky" prices or some sort of rote behavior or "irrational exuberance" in asset prices or all of these. What Keynes and Hayek in the 1930s and Phelps in the 1960s understood is that there may be a problem with expectational equilibrium and it need not be sticky prices or irrationality but mainly the

6. I would add that in relatively recent work (Phelps, 1994) I simply treat every shock as de novo, so the state it brings was fully unanticipated. Obviously this treatment is at odds with rational expectations. However, I regard the implications of that model to fit more closely with the behavior of national economies than do the models that invoke a stochastic stationary state with no room for parametric shifts.

ever-imperfect knowledge of the structure of the economy and the attempt of purposeful market participants and policy makers to cope with it.[7]

If rational expectations equilibrium and its doppelganger predeterminacy must be regarded as inapplicable to the modern economy, the profession needs to embark on its own voyage of discovery. The present book is devoted to setting out a fresh approach, one that is neither rational-expectationist nor behavioralist.

The authors of this book show that if we want to do coherent macroeconomic theorizing about a modern economy we are going to have to allow in our models for non-routine decision making and unforeseeable changes in the social context within which individuals make decisions. How do we build such a *theory* for modern economies?

The authors devote most of the book to developing such a theory, which they dub "imperfect knowledge economics." This economics builds in mathematical microfoundations of aggregate outcomes and yet it allows for non-routine ways in which market participants might alter the way they deploy resources. The remarkable feature of these imperfect knowledge models is that, while they do not assume away non-routine activities, they nevertheless generate implications that allow an economist to compare empirically the performance of alternative explanations of outcomes.

How is this done? The key point is that imperfect knowledge economics focuses on *change* and looks for *qualitative* regularities, not quantitative ones. The authors' models impose qualitative restrictions on the way forecasting strategies are revised. While placing enough structure on an economist's model, these restrictions are general enough to be compatible with a myriad of ways in which market participants might revise their views of the future. Moreover, these restrictions recognize that *sharp* forecasts of what an individual will do are beyond the reach of any economic analysis of *modern* economies.

This approach resolves Knight's and Keynes's problem of how to reconcile the use of probability theory in modeling decisions under uncertainty. As Knight and Keynes recognized, neither the actors nor the economist-modeler knows the probability distribution of outcomes. The key innovation of the authors is to model the change across time in these distributions and in a purely qualitative way (the authors refer to these as "partially predetermining restrictions") rather than to model the probability distribution at each point in time.

7. Leijonhufvud (1968) also attributes this view to Keynes, and he identifies himself with that view. I should add that, although I participated in the New Keynesian venture in the 1970s to rewrite Keynesian economy on the basis of rational expectations coupled with non-synchronous wage/price setting, my heart was always with the model in which wages and prices were continually being revised.

The three-decade-long debate between the neoclassical and "Keynesian" schools over whether prices are sticky or flexible appears to be a mere distraction. In the context of the foreign exchange market, the authors show that, with incomplete knowledge, long swings in real exchange rates do not depend on whether prices are sticky or flexible. Rather, they arise from the imperfection of knowledge concerning the structure of the economy and market participants' attempts to cope with it. Moreover, in contrast to recently fashionable behavioral models, the authors' explanation of swings does not abandon the long tradition in economics that individuals behave in largely rational, or reasonable, ways.

Remarkably, once the authors allow for imperfect knowledge on how fundamentals influence the exchange rate, long swings can arise even if all market participants' diverse forecasting strategies depend solely on the macroeconomic fundamentals. It would not be surprising, therefore, if it is later found that a similar mechanism generates swings in overall business activity. (This possibility suggests that if modified by the authors' imperfect knowledge framework, my models of "structural slumps" would generate not a monotone shift from the initial steady state to the new one but rather a cyclical transition.)

In the conventional conception, as I pointed out above, market outcomes are mere vibration around a steady state path. Swings are viewed as anomalous and puzzling. Once imperfect knowledge is placed at the center of the analysis, swings arise as part of the discovery process of how prices are related to macroeconomic fundamentals.

The authors' imperfect knowledge economics sees the modern economy as possessing bounded instability around historical benchmark values, which themselves may be evolving over time. The importance of historical benchmarks in characterizing individual behavior and aggregate outcomes was emphasized by almost all important early modern economists: Wicksell, Keynes, and Tobin, who built on this in his work on "behavior toward risk." (I imagine this view will be found to link well to my own work on movements of the medium-term natural unemployment rate.)

Another hallmark of the imperfect knowledge view is its qualification of fixed policy rules. The necessary point is that the optimum rule is not the same from one structure of the economy to another. As with the rest of macroeconomics, the issues have to be rethought in a way that makes the ever-imperfect knowledge of market participants and policymakers an integral part of the analysis.

I had the great good fortune in the 1960s to initiate the profession's work on plausible microfoundations for macroeconomic modeling, taking into account the knowledge and the information that the micro-actors could reasonably be supposed to have—a revolutionary movement, it seems. Unfortunately, the rational expectations models, appearing in the 1970s,

sidestepped the problem of expectations formation under uncertainty by blithely supposing that the model's actors (tellingly dubbed "agents") knew the "correct" model and the correct model was the analyst's model—whatever that model might be that day. The stampede toward "rational expectations"—widely thought to be a "revolution," though it was only a generalization of the neoclassical idea of equilibrium—derailed the expectations-driven model building that had just left the station. In the end, this way of modeling has not illuminated how the world economy works. Happily for me and, I believe, for the profession of economics, this deeply original and important book gives signs of bringing us back on track—on a road toward an economics possessing a genuine microfoundation and at the same time a capacity to illuminate some of the many aspects of the modern economy that the rational expectations approach cannot by its nature explain.